CHAPTER III.—THERMOMETER.

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2. F. ,, C.
3. C. ,, F.
4. F. ,, C. differences
5. C. ,, F. ,,

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4. ,, ... ,, ... English.
5. ,, ... Altitude ... metric.
6. ,, ... ,, ... English.
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CHAPTER V.—HYGROMETRY, RAIN, AND EVAPORATION.

1. Vapour-tension to 0·1 C. from − 30° C. to + 101° C.
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3. Boiling-point (from 680 mm.—800 mm.) ... metric.
4. ,, ,, ,, ... English.
5. Vapour-tension about 100° C. ... ... metric.
6. ,, ,, 212° F. ... ... English.
7. Weight of water in cubic metre of air ... metric.
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CHAPTER VI.—WIND.

1. Lambert's formula.
2. Natural tangents.
3. Kilometres per hour to metres per second.
4. Metres per second ,, kilometres per hour.
5. Miles per hour ,, metres per second.
6. Metres per second ,, miles per hour.

CHAPTER VII.—MAGNETISM AND ELECTRICITY.

1. English mag. units to C.G.S. units.
2. C.G.S. ,, Eng. mag.

Weight and Mass.

THE review of Kennedy's "Mechanics of Machinery" in NATURE, December 29, 1887 (p. 195), strikes at least one responsive chord on this side of the world. There are some questions in reference to the nomenclature of dynamics which "will not down" until they are "downed" by a convention or agreement between those who have to do with the theory of mechanics and those who have to do mostly with practice, and in this some concessions will doubtless be necessary on both sides. While in hearty sympathy with much that the reviewer says in his discussion of dynamical terms (the book under notice I have not yet seen), I wish to dissent from and to protest against one of his leading propositions.

It must be admitted that in the "vernacular" the word pound is used in two distinct senses—that is, as a unit of force and a unit of mass. Authors of mathematical treatises have sometimes, and perhaps unconsciously, ignored the latter meaning, and at other times have failed to recognize the former.

The proposition of the reviewer is to eliminate the word *mass* altogether and to use weight in its stead. To accomplish this he is obliged to use the word *weight* as meaning what is now generally expressed by the word *mass*. This, it seems to me, would be a grave error. Is it not true that *weight*, as understood by both the "learned and the unlearned" always carries with it the idea of force, the force of attraction between the earth and the particular body under consideration? And is it not also true that there are many problems in the work of the practical engineer in which *mass*, in the ordinarily accepted sense, is the essential element, rather than weight, in the ordinarily accepted sense? In short, in my judgment, the engineer *does* require the word "mass," and he also needs the word "weight." It is a misfortune when one word must be used to mean two entirely different things (as is the case of the word "pound"), and we ought to congratulate ourselves that we have the words "mass" and "weight" so commonly and generally used to represent two distinct ideas. To discard one of them and force the other into its place would be to introduce confusion rather than order. To satisfy the requirements of both mathematical or theoretical and practical convenience I have been accustomed to use the following:—

The word *pound* is used in two senses; it may mean a unit of mass or a unit of force. It is always easy by the context to tell in which sense it is used.

As a unit of force it *has not yet been accurately defined*, but it means, in general a force equal to the attraction between the earth and a mass of one pound. As this attraction varies slightly, the pound as a unit force cannot be regarded as absolutely constant, but is sufficiently so for practical purposes.

When, by a convention of authorities, the conditions under which this attraction is accepted as equal to one pound are prescribed, it will become an invariable unit.

There are in the English system two units of force, the poundal and the pound. There are also two units of work, the foot-poundal and the foot-pound; each is the work done by the corresponding unit of force working through a distance of one foot.

The ordinary equations of dynamics, when the foot-pound-second units are used, give results in poundals or foot-poundals, which may at once be reduced to pounds or foot-pounds.

The above is open to the objection that the pound as a unit of force is not constant, but the remedy for this is indicated, and the errors introduced are of no moment in "practice."

To lessen the confusion somewhat, I have often used, in writing, the symbol *lb.* to represent the unit of *mass*, and the word *pound* that of force. In my own experience the adoption of these definitions has greatly facilitated the work of students.

I entirely agree with the criticisms made upon the equation so constantly appearing, $w = mg$. To the learner it is generally "confusion confounded," and I would cheerfully join in a "boycott" against it. T. C. MENDENHALL.

Rose Polytechnic Institute, Terre Haute, Indiana,
U.S.A., January 26.

ONCE more Prof. Greenhill devotes a large portion of a review to emphasizing and insisting on his peculiar, and I may say extraordinary, mode of regarding the meaning of elementary terms (see NATURE, February 16, p. 361; also December 29, 1887, p. 195).

One must assume, therefore, that these views are regarded by him as useful and conducive to clearness.

I find it difficult to express strongly enough my entire dissent from such a proposition without being apparently impolite.

That engineers are entitled if they see fit to employ as their third fundamental standard a standard of force rather than one of mass, I admit. I do not think the plan satisfactory or clear, but there are temptations towards it, and perhaps no very serious objections. My own experience of engineering students is, however, that they are beautifully uncertain whether to put g into the numerator or the denominator of a new expression, or whether to leave it out altogether; and that they generally get over the difficulty either by asking where it must go, or by seeing which plan will give an answer of most reasonable magnitude. The real rule on engineers' principles would be to put g somewhere into the expression for any quantity with which gravity has nothing to do, and to leave g out whenever gravity is primarily concerned.

But, irrespective of this standing and well-known controversy, Prof. Greenhill's attempt to simplify matters does indeed make confusion worse confounded. He says that in the vernacular the term "weight" does not mean the force with which the earth pulls a body, but does mean the body's mass or inertia.

What kind of "vernacular" can he be thinking of?

Ask any ordinary member of the British public what he or she means by the "weight" of a thing, and you will get answers such as "its heaviness," or "its heft," or the "force required to lift it," or "the difficulty of raising it," or "the pull up you must give it," or any number of such replies; but if he ever got the answer, "I mean the mass of the body, in other words its inertia, a measure of the quantity of matter the body contains," surely he would not be satisfied with this as a fair specimen of the vernacular, but would rather regard it as one of those answers so frequently given to examiners—the product of a mind so tortured by instructors that its common-sense and vernacular are completely atrophied. OLIVER J. LODGE.

The Composition of Water

Two days after the publication of my letter in NATURE (p. 390), on the composition of water, I received the Manchester

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Report of the British Association, in which (p. 668) further experiments by Dr. A. Scott are reported. Dr. Scott has succeeded in reducing the amount of nitrogen present as impurity to 1 part in 15,000, and the ratio of hydrogen to oxygen which he calculates from the newer and more accurate experiments is 1·996 or 1·997 to 1·000. This ratio agrees very well with that deduced by me from the older experiments, but is considerably higher than the ratio previously adopted by Dr. Scott, and quoted by Prof. Thorpe in his article on the composition of water.
SYDNEY YOUNG.

University College, Bristol.

ON THE DIVISORS OF THE SUM OF A GEOMETRICAL SERIES WHOSE FIRST TERM IS UNITY AND COMMON RATIO ANY POSITIVE OR NEGATIVE INTEGER.

> "Nein! Wir sind Dichter."[1]
> —*Kronecker in Berlin.*

A REDUCED Fermatian,[2] $\frac{r^p - 1}{r - 1}$, is obviously only another name for the sum of a geometrical series whose first term is unity and common ratio an integer, r.

If p is a prime number, it is easily seen that the above reduced Fermatian will not be divisible by p, unless $r - 1$ is so, in which case (unless p is 2) it will be divisible by p, but not by p^2.

This is the theorem which I meant to express in the footnote to the second column of this journal for December 15, 1887, p. 153, but by an oversight, committed in the act of committing the idea to paper, the expression there given to it is erroneous.

Following up this simple and almost self-evident theorem, I have been led to a theory of the divisors of a reduced Fermatian, and consequently of the Fermatian itself, which very far transcends in completeness the condition in which the subject was left by Euler (see Legendre's "Theory of Numbers," 3rd edition, vol. i., chap. 2, § 5, pp. 223-27, of Maser's literal translation, Leipzig, 1886),[3] and must, I think, in many particulars be here stated for the first time. This theory was called for to overcome certain difficulties which beset my phantom-chase in the chimerical region haunted by those doubtful or supposititious entities called odd perfect numbers. Whoever shall succeed in demonstrating their absolute non-existence will have solved a *problem of the ages* comparable in difficulty to that which previously to the labours of Hermite and Lindemann (whom I am wont to call the Vanquisher of PI, a prouder title in my eyes than if he had been the conqueror at Solferino or Sadowa) environed the subject of the quadrature of the circle. Lambert had proved that the Ludolphian[4] number could not be a fraction nor the square root of a fraction. Lindemann within the last few years, standing on the shoulders of Hermite, has succeeded in showing that it cannot be the root of any algebraical equation with rational coefficients (see Weierstrass' abridgment of Lindemann's method, *Sitzungsberichte der A.D.W. Berlin*, Dec. 3, 1885).

It had already been shown by M. Servais ("Mathesis," Liège, October 1887), that no one-fold integer or two-fold odd integer could be a perfect number, of which the proof is extremely simple. The proof for three-fold and four-fold numbers will be seen in articles of mine in the course of publication in the *Comptes rendus*, and I have been able also to extend the proof to five-fold numbers. I have also proved that no odd number not divisible by 3 containing less than eight elements can be a perfect number, and see my way to extending the proof to the case of nine elements.

How little had previously been done in this direction is obvious from the fact that, in the paper by M. Servais referred to, the non-existence of three-fold perfect numbers is still considered as problematical; for it contains a "Theorem" that if such form of perfect number exists it must be divisible by fifteen: the ascertained fact, as we now know, being that this hypothetical theorem is the first step in the *reductio ad absurdum* proof of the non-existence of perfect numbers of this sort (see NATURE, December 15, 1887, p. 153, written before I knew of M. Servais' paper, and recent numbers of the *Comptes rendus*).

But after this digression it is time to return to the subject of the numerical divisors of a reduced Fermatian.

We know that it can be separated algebraically into as many irreducible functions as there are divisors in the index (unity not counting as a divisor, but a number being counted as a divisor of itself), so that if the components of the index be $a^\alpha, b^\beta, c^\gamma, \ldots$ the number of such functions augmented by unity is

$$(\alpha + 1)(\beta + 1)(\lambda + 1). \quad . \quad . \quad . \quad .$$

All but one of these algebraical divisors, with the exception of a single one, will also be a divisor of some other reduced Fermatian with a lower index: that one, the core so to say (or, as it is more commonly called, the irreducible primitive factor), I call a cyclotomic function of the base, or, taken absolutely, a cyclotome whose index is the index of the Fermatian in which it is contained.

It is obvious that the whole infinite number of such cyclotomes form a single infinite complex. Now it is of high importance in the inquiry into the existability of perfect numbers to ascertain under what circumstances the divisors of the same reduced Fermatian, *i.e.* cyclotomes of different indices to the same base can have any, and what, numerical factor in common. For this purpose I distinguish such divisors into superior or external and inferior or internal divisors, the former being greater, and the latter less, than the index.

As regards the superior divisors, the rule is that any one such cannot be other than a unilinear function of the index (I call $kx + 1$ a unilinear function of x, and k the unilinear coefficient) and that a prime number which is a unilinear function of the index will be a divisor of the cyclotome when the base in regard to the index as modulus is congruous to a power of an integer whose exponent is equal to the unilinear coefficient.

As regards the inferior divisors, the case stands thus. If the index is a prime, or the power of a prime, such index will be itself a divisor. If the index is not a prime, or power of a prime, then the only possible internal divisor is the largest element contained in the index, and such element will not be a divisor unless it is a unilinear function of the product of the highest powers of all the other elements contained in the index.

It must be understood that such internal divisor in

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[1] Such were the pregnant words recently uttered by the youngest of the splendid triumvirate of Berlin, when challenged to declare if he still held the opinion advanced in his early inaugural thesis (to the effect that mathematic consists exclusively in the setting out of self-evident truths,—in fact, amounts to no more than showing that two and two make four), and maintained unflinchingly by him in the face of the elegant raillery of the late M. Duhamel at a dinner in Paris, where his interrogator—the writer of these lines—was present. This doctoral thesis ought to be capable of being found in the archives of the University (I believe) of Breslau.

[2] The word Fermatian, formed in analogy with the words Hessian, Jacobian, Pfaffian, Bezoutiant, Cayleyan, is derived from the name of Fermat, to whom it owes its existence among recognized algebraical forms.

[3] I find, not without surprise, that some of the theorems here produced, including the one contained in the corrected footnote, have been previously stated by myself in a portion of a paper "On certain Ternary Cubic Form Equations," entitled "Excursus A—On the Divisors of Cyclotomic Functions" (*American Journal of Mathematics*, vol. ii., 1879, p. 357) the contents and almost the existence of which I had forgotten: but the mode of presentation of the theory is different, and I think clearer and more compact here than in the preceding paper; the concluding theorem (which is the important one for the theory of perfect numbers) and the propositions immediately leading up to it in this, are undoubtedly not contained in the previous paper.

I need hardly add that the term *cyclotomic* function is employed to designate the core or primitive factor of a Fermatian, because the resolution into factors of such function, whose index is a given number, is virtually the same problem as to divide a circle into that number of equal parts.

[4] So the Germans wisely name π, after Ludolph van Ceulen, best known to us by his second name, as the calculator of π up to thirty-six places of decimals.

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